Reply to the two referees:

It is no easy task to give fully covering response to the many comments and objections raised by the two referees. Considering them (now for quite a long time) two major reasons come to mind:

1. Many points made in our paper have not been motivated and explained well enough. These were useful comment by the referees as they will help to improve the readability of the paper.

2. To some extent, we are speaking different languages, with obvious misunderstandings as a consequence. Of course, such comments also falls back on the paper in the sense of us not having explained the approach well enough, and having using concepts in an econometric context which give strong connotations in an economic context but with different meanings. For a useful rewiev of such concepts see Framroze (2008) in this journal. We suggest that a new section on the methodological approach called "The 'theory first' and the 'data first' approach" is added to the paper.

Since one of the great advantages of this journal is that it encourages discussion and debates we will use this opportunity to discuss two fundamentally different approaches to empirical economics which, somewhat loosely, could be labelled 'theory comes first' versus 'data come first'. The latter (used here) is basing its scientific foundation on strict statistical/econometric principles, selecting the data based on broad economic relationships (demand and supply functions, etc.) so that the final empirical results might be able to discriminate between several competing specific theory models, or rather hypotheses derived from different theories. This allows us on one hand to discriminate between competing theory models and, on the other hand, to detect new evidence not yet formulated as a prior hypothesis. The former approach is based on the assumption that the basic economic mechanisms can be pre-specified, i.e. that we know which the exogenous forces are, how interventions have affected the system, etc. Econometrics in this case play the subordinate role of getting estimates of the economic parameters assumed to be empirically relevant from the outset. Such an approach, however, runs the risk of producing empirically irrelevant and misleading results as demonstrated by Juselius and Franchi (2007) in this journal and by the many papers in this special issue. Even more important, this approach will almost by construction be less open to signals in the data suggesting that the assumed

true mechanisms are incorrect. Also, editorial policy frequently seems to be hostile towards accepting papers demonstrating flaws in published papers (*$\mathcal{E}$conomics* being an appreciated exception).

No doubt the 'theory comes first' approach has been and still is the preferred way of doing research in economics. However, there are some methodological problems associated with the 'Walrasian theory comes first' as for example Hoover (2006) has pointed out :

"*The Walrasian approach is totalizing. Theory comes first. Empirical reality must be theoretically articulated before it can be empirically observed. There is a sense that the Walrasian attitude is that to know anything, one must know everything. " .... "There is a fundamental problem: How do we come to our a priori knowledge? Most macroeconomists expect empirical evidence to be relevant to our understanding of the world. But if that evidence only can be viewed through totalizing a priori theory, then it cannot be used to revise the theory."..."The Marshallian approach is archaeological. We have some clues that a systematic structure lies behind the complexities of economic reality. The problem is how to lay this structure bare. To dig down to find the foundations, modifying and adapting our theoretical understanding as new facts accumulate, becoming ever more confident in out grasp of the super structure, but never quite sure that we have reached the lowest level of the structure.*"

Based on numerous empirical analyses from a variety of different countries (small, medium sized, large, open, closed, regulated, deregulated, etc.) there is a mounting evidence that 'the Walrasian approach' frequently fails to demonstrate its relevance, if the goal is to understand the empirical variation of our macro data. It seems unlikely that today's most pressing economic problems, such as the present financial crises, can be adequately addressed by further elaborating the economists' standard tool kits including representative agents and model based rational expectations.[1] The effect of globalization on domestic economic policy, international governance and regulation, the effect of capital deregulation on domestic economies, how to guide and assist under-developed countries, etc. are other examples where the 'theory-comes-first' approach does not seem to have provided us with empirically useful advice.

Let us assume for a moment that economists, say as a result of the present

[1] The ultimate implication of these models is that a few structural (constant) parameters describing technology and preferences are sufficient to explain our complicated reality.

financial crises, will start looking for a paradigmatic change in their understanding of the empirical reality. How should it come across? Our answer is simply to learn from the data in a systematic and structured way. To do this as efficiently as possible, we argue that the order of integration, rather than a structural parameter, should be considered an empirical approximation, measuring the degree of persistent behavior in a variable or relation (see Juselius, 2008 for a detailed discussion). Because the unrestricted VAR is essentially a convenient reformulation of the information in the data (Hendry and Mizon, 1993, Juselius, 2006, Chapter 3), the VAR approach seems an obvious platform from which to take off. If valid statistical principles are strictly adhered to when testing restrictions on the VAR, then the final parsimonious model should provide a set of empirical facts (not stylized!) which a theoretical model should be able to explain in order to claim empirical relevance. In this sense, the VAR model can be thought of as providing confidence bands (broadly defined) within which the theory model should fall.

Many economists would argue that the quality of economic data is too low for such a possibility. We agree that economic time series data seldom correspond very closely to the theoretical concepts of a theory model (prices, income, money, etc. in a theory model versus the multitude of different measurements CPI, PPI, PY, GDP, GNE, DI, M1 M2, M3, etc. that can be chosen in an empirical analysis). Though macro data are contaminated with measurement errors, the latter may not be of great concern unless they are systematic and cumulate to nonstationarity. Another point is that the theoretically correct measurements are not observable and, hence, cannot be used by politicians and decision makers to react on. The forecasts, plans and expectations that agents base their decisions on are the observed data, however imperfect they are. Hence, in our view, we have to understand the mechanisms having generated these data.

If one accepts this view and take macroeconomic data seriously, it comes often as a surprise how informative they are. The biggest hurdle in learning from data is the (almost irresistible) urge to impose too many economic priors on the statistical model, in spite of them being against the information in the data. To start from the idea that we know what the empirical model should tell us and then insisting that the reality should behave accordingly is a receipt for deep frustration, which too often leads the researcher to begin 'torturing' the data until they confess.

Rather than insisting that we can pre-specify *the* correct economic model and that agents make decision knowing this correct model and hence the

correct information set, we might instead accept the obvious fact that agents do not know the right model, nor the right variables, that agents change their view as they learn more. Furthermore, if we add that agents, in view of their imperfect knowledge, are risk averse and myopic, then we will end up with a theoretical framework that has shown to be empirically relevant (even astonishingly so see Frydman and Goldberg, 2007, Johansen, Juselius, Frydman and Goldberg, 2008, Frydman, Goldberg, Johansen, and Juselius, 2008, Juselius, 2008). However, the theory of imperfect knowledge economics (IKE) avoids the pitfall of assuming constant 'structural' parameters over time, as it is inherently inconsistent with imperfect knowledge expectations. Instead, IKE gives qualitiative predictions which, though testable, are looser than, say those derived from a Rational Expectations (RE) model. Exactly because the IKE theory does not force the empirical model into the straightjacket of a prespecified model, it needs the partnership of a strong empirical methodology such as the cointegrated VAR. Because, the aim of the cointegrated VAR methodology is to describe the complicated empirical reality, not a prespecified theoretical model, it needs to be combined with a theory that can guide in the search for empirically relevant mechanisms without hampering the search.

For example, the IKE theory predicts that financial expectations based on imperfect knowledge is likely to drive prices away from long-run benchmark values (equilibria) rather than towards them for extended periods of time. In the CVAR approach these pronounced movement away from long-run benchmark values are often statistically indistinguishable from unit root processes with the consequence that statistical testing suggests more stochastic trends than the RE based models can explain. See for example the applications to prices, exchange rates, and interest rates in Juselius, 2008, Johansen, Juselius, Frydman, and Goldberg, 2008, Juselius and MacDonald, 2004, 2007. The latter demonstrate empirically that the persistent movements in real exchange rates during periods of currency float (the long swings puzzle) are generally empirically I(1), even near I(2) as is the real long-term interest rate spread. [2]

To argue that we would be able to pre-specify in an IKE world how the Balassa-Samuelson effect and the increased product market competion should

[2] This is in fact consistent with one of the predictions of the IKE theory which seems to provide a resolution to the 'PPP puzzle' (see Frydman, Goldberg, Johansen, and Juselius, 2008).

have affected the Spanish wage and price setting in this period (knowing what is important and what is not, which variables are exogenous, which are endogenous) would no doubt be a little heroic. The best one can hope for, in our view, is to uncover the most significant empirical facts, not stylized facts, by characterizing data within the broad framework of (possibly) relevant theory models, thereby uncovering the basic features an empirically relevant theory model should be able to explain. Thus, to some extent, The CVAR approach switches the role of theory and statististical analysis in the sense of rejecting the privileging of *a priori* economic theory over empirical evidence (Hoover, Johansen and Juselius, 2008).

The idea of this special issue was exactly to use econometrics to find out whether what we believe is empirically relevant, or not. I cite from the purpose of this special issue:

Background and motivation: Econometrics is often used passively to provide the economist with some parameter estimates in a model which from the outset is assumed to be empirically relevant. In this sense, econometrics is used to illustrate what we believe is true rather than to find out whether our chosen model needs to be modified or changed altogether.

The econometric analyses of this special issue should take its departure from the latter more critical approach. We would like to encourage submissions of papers addressing questions like whether a specific economic model is empirically relevant in general or, more specifically, in a more specific context, such as in open, closed, deregulated, underdeveloped, mature economies, etc. For example, are models which were useful in the seventies still relevant in the more globalized world of today? If not, can we use the econometric analysis to find out why this is the case and to suggest modifications of the theory model?

We encourage papers that make a significant contribution to the discussion of macroeconomics and reality, for example, by assessing the empirical relevance of influential papers, or the robustness of policy conclusions to econometric misspecification and the ceteris paribus clause, or by comparing different expectations's schemes, such as the relevance of forward versus backward expectations and of model consistent rational expectations versus imperfect/incomplete knowledge expectations, etc.

Based on this general background we will now comment on the points raised by the referees, some of which show that our argumentation has been weak or incomplete (and we agree they need to be improved) others which we believe are based on a misunderstanding of the purpose of the paper.

Major points:

1. Both referees are unhappy with section 3 'Theoretical background'. I believe part of this is because a theoretical background is usually assumed to contain a much more stringently derived theoretical model, rather than the fairly loosely stated hypothetical relationships given in this section. In particular, the introduction of the general framework at p.6 has generated many disapproaving comments as the empirical investigation does not fully cover all aspects of this theoretical framework. We propose to remove this part from the paper and rephrase Section 3 as 'Testable hypotheses on wage and price setting behaviour'. Referee 2 suggests that we should skip most of the material in Section 3 and go directly to the empirical analyses. However, this would make the empirical approach seem completely inductive, which it is not. We are using what is sometimes called abductive inference: The relationships in Section 3 have been previously found in other countries' data and in that sense qualifies as prior hypotheses in the present data analysis. One important idea of this approach is that, by using the same methodological approach on different economies (studying the empirical reality through the same glasses) one can learn about similarities and dissimilarities and, hence, about the effect of regulation/deregulation, different economic regimes, etc. Each such study is likely to produce some new results that generate new hypotheses which can be formulated and tested as a prior hypothesis in the next study.

2. Both referees have concerns regarding the choice of our four regimes, expressing doubts about the objectivity with which they are defined and interpreted, and in particular with the introduction of step (shift) dummies as a way of modelling these regimes. Here, some clarifications are needed: The regimes have been defined based on statistical testing in a FIML framework using the following procedure (described in detail in Juselius, 2006): An unrestricted VAR model is first estimated and checked for outlying observations in Δx_t. In case these correspond to known reforms, interventions, policy regime changes, etc. they will be subject to further scrutiny. In particular, an extraordinary impulse in Δx_t corresponds to a level shift in x_t, suggesting that the large shock might coincide with a shift in the equilibrium mean, $E(\beta' x_t)$. This can be formulated as a testable hypothesis. In the present application, the model identified outlying observations in 1986:1, 1992:3, 1999:1, 1995:1, 2001:1. Of these, based on tests for long-run exclusion, only the first three were found to have significantly changed the equilibrium mean. That these changes in the equilibrium mean are indeed highly significant can be inferred

from Table 3 p.17.

3. Both referees have concerns about modelling the exchange rate as an endogenous variable and relatedly about the specific-to-general approach of extending the set of variables in our VAR model. See discussion above on the role of real exchange rate and the long-term interest rate in the model.

3.1. The role of the real exchange rate and the long-term bond rate in the VAR model: As correctly pointed out by the referees, the nominal exchange rate was for practical reasons fixed after 1995, implying that after that date the real exchange rate is essentially equal to relative prices. However, the constructed real exchange rate would still correctly measure Spanish competitiveness, suggesting that the cointegration results should be valid also after fixing the peseta. Similar arguments can be said for the long-term bond rate. However, the adjustment dynamics may not be unaffected by a regime shift and I believe this is a valid reason for concern by the referees. As a matter of fact, standard theory would predict that nominal exchange rates adjust during a floating regime and prices during a fixed regime. The IKE theory, however, suggest that even during a period of currency float goods prices are adjusting to (persistent) deviations from PPP. Thus, independently of whether we have a fixed or a flexible exchange rate regime prices will be adjusting and pricing-to-market will be the dominant price setting mechanism. The Spanish real exchange rate has exhibited persistent long swings in the period up to 1995 and cointegration relation 4 shows that prices are indeed adjusting to real exchange rates while cointegration relation 5 shows that real exchange rates and the long-term interest rate are cointegrated (together with inflation and unemployment rate). Thus, the concerns raised by referee 1 that there is a shift in exogeneity around 1995, while theoretically relevant in an RE world, is not likely to be relevant in an IKE world and the empirical results seem to suggest than the latter is empirically more relevant.

3.2. Sequential identification of cointegration relations: referee 1 argues that it would be preferable to go straight to the full model and keep exchange rates and the bond rate as exogenous. This however would be against the idea of 'letting the data speak freely'. Our point is that the more prior economic assumptions are allowed to influence the VAR specification from the outset, the less likely it is that we will find out whether our prior beliefs are empirically correct or not. The sequential approach starting with a smaller model and adding variables is indispensable when the total number of variables is fairly large. Because the cointegration property is invariant to changes in the information set, one can gradually learn about which variables

cointegrate, and also slowly build up one's intuition on the importance of the ceteris paribus clause of the underlying theoretical models. Also, a sequential approach provides useful information about the endogeneity/exogeneity status (pulling and pushing forces) and how it changes when adding new variables. For example, when adding real exchange rate to model 1, we might have found that the rank did not change. This would have implied that real exchange rate had been affected by at least one stochastic trends not shared by the domestic variables, or we might have found (as we did) that the rank increased by one implying that the domestic variables had fully absorbed the shocks from abroad. Of course, the order of selection may easily change the final cointegration relations in the following sense if x1 and x2 are cointegrated and x2 and x3 are cointegrated, then x1 and x3 are also cointegrated. But this is more a matter of taste as in the full model the adjustment dynamics, α, will take account of any rotation of the long-run structure β.

As argued above, if the real world were well described by the assumption of RE behavior, we should have tested (or modeled) a shift in exogeneity by allowing the adjustment coefficients to change (though it would possibly have been too econometrically due to the high dimension of the system), whereas in an IKE world it makes perfectly sense to study the feedback impact from the domestic economy on the behavior of the real exchange rate, even though nominal exchange rate has been fixed since 1995.

To meet the concerns by the referees we suggest that a new Table with the estimated long-run matrix $\Pi = \alpha\beta'$ for the full set of variables is included in the paper. This should make the effect of the real exchange rate and the long-term interest rate on the domestic variables more transparent. The short-run adjustment dynamics in Table 5 is not necessarily robust to changes in the conditioning set. We have therefore re-analyzed the short-run model assuming that the real exchange rate and the long-term interest rates are exogenous (all other equations wer conditioned on current and lagged changes in the two variables). The current changes were both excludable from the system with p-values 0.5 and 0.2 respectively. This was not surprising as the residual correlations were generaly small in our VAR system. Thus, treating real excahnge rates and the long-term bond rate as exogenous would is not likely to change any of the results, but would hide the fact that they are to some extent influenced by the domestic wage, price and unemployment dynamics.

4. Is the information set large enough? Referee one would like us to

include a variable measuring exogenous external demand and possibly also the Spanish-German interest rate spread. But how would we know from the outset that the 'new' variables are empirically exogenous? Spain is a large European country with large export and import sectors and the feed-back effects cannot be assumed to go just one-way. But, it seems quite likely that the effect of foreign demand on the Spanish economy should primarily be an indirect one via the Spanish GDP (which is included in our analysis in terms of GDP/emplyment) and the effect of the German interest rate should also be indirect via the Spanish interest rate (which is included). Of course one could ask why is not European inflation rate included? why not imports and exports, etc.? Unfortunately one drawback of the VAR model is that it becomes increasingly cumbersome to analyze as the number of variables increase. A 7-dimensional system as the present one is probably close to the maximum for what can be adequately handled. This means that one needs to proceed slowly, choosing the most relevant (explanatory) variables for those variables we are primarily interested in, implying that some of the VAR variables will be partially determined, for example the real exchange rate, the real bond rate, and the productivity variable. Ideally they should be analyzed in separate VAR models and the results combined into a more complete model (see Juselius, 2006, Chapter 19 for a discussion and an illustration of such an approach). This, however, is outside the scope of the present paper.